

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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No. 3.

Admiral MANNERS, President, in the Chair.

Rev. R. H. Blair, Worcester ;
E. Clodd, Esq., Edmonton ;
Capt. Haig, R.E., Poonah ;
Howell Jeffreys, Esq., Balliol College, Oxford ;
James Knight, Esq., Glasgow ;
Lt.-Gen. Lane, Sandown, Isle of Wight ;
Rev. J. Saywell, Bromsgrove ;
A. F. Smith, Esq., Training College, Borough Road ;
E. J. White, Esq., Observatory, Melbourne ; and
Lieut. Wither, Tangier Park, Basingstoke,

were balloted for and duly elected Fellows of the Society.

On the Solar Eclipse of August 18th, 1868.

By Warren De La Rue, Esq.

Under date of November 9th, 1868, Major Tennant sent to me, from Calcutta, enlarged paper copies, a little over two inches in diameter, of each of six photographs he had obtained of the eclipse; also a negative photographic enlargement of the Great Horn (which I call A), about nine times the size of the original negative; and, lastly, a photograph of a drawing of this great prominence made by Mr. James on a scale about three times the size of fig. 7.

With reference to these photographs Major Tennant makes the following remarks:—

“In each of the photographs the corner next the Great Horn (A) is very nearly where the *apparent* vertex is in an inverting telescope. The inclination of the wire (in Nos. 1 and 2) was $39^{\circ} 15'$ to the parallel, and the position angle of the horn is 75° .”

I suggested to Major Tennant, before he left England, that it would be advantageous, in order to refer the position of the prominences to the Sun's centre, to take enlarged pictures on glass, and to etch them according to the plan I showed him, and which I had used for the eclipse of 1860, and obtained, by the electrotype process, the copper multiples from which were printed Plates xiii., xiv., xv., &c., in the *Philosophical Transactions*, 1862. In reference to this suggestion, Major Tennant says:—"The large pictures were thin and poor, and there was no use in treating them like yours by etching; for the real thing would be better shown by a distribution of the transparencies, so as to make them generally accessible."

Major Tennant has accordingly sent eight sets of transparent copies on glass of his eclipse-pictures to the Astronomer Royal for distribution; one set of these is for me, and when they come into my hands* I intend to try and procure from them etched plates like those which I made of the eclipse of 1860, as I have more leisure than Major Tennant could command, and also appliances for doing so which he does not possess. In the meantime, however, I have worked on the paper copies, and although the results I have obtained must be considered merely as first approximations, I venture to think that they may have some interest for Astronomers, inasmuch as they afford data for referring the prominences seen at different stations to a meridian drawn through the Sun's centre, thus offering facilities for a complete study of the whole phenomena observed at the various stations along the line of the central eclipse.

The only prominence visible *throughout* the totality at Major Tennant's station at Guntoor was the Great Horn (position-angle 75° in reference to the Moon's centre). Prominence G first became visible at the epoch of No. 5 photograph; at the epoch of No. 4 only the Great Horn, A, was depicted. Prominence C is completely covered in No. 3, and prominence B partially so, as well as the soft light indicated by dotted lines in the diagram, close to B towards the north, position angle 116° to 125° , height above the Moon's limb $1' 41''$. The outline of this soft light is not changed in the interval between 2 and 3, and it cannot, on account of its well-defined form, have arisen from light reflected from the photosphere by the Sun's atmosphere. Another patch of light, with a softened but characteristic branching outline, is visible between the prominences B and C, position angle 133° to

* I received these photographs from the Royal Observatory on Jan. 22d, while this paper was passing through the press. They contain much more detail than is visible in the paper copies; for example, in No. 1, a band of low prominences can be seen in the midst of the glare, hereinafter alluded to, extending from position angle 96° right up to prominence B. In No. 2 is a very curious group of faint prominences K making up half an ellipse, from position angle 65° to 72° , the minor axis of the ellipse extending about $40''$ above the Moon's limb. These pictures are extremely interesting, and, in my opinion, must be considered as eminently successful results.—Jan. 23, 1869. W. D. L. R.

139°, height above the Moon's limb 2' 27". Similar appearances are indicated in my eclipse-pictures of 1860, and form part of the appendages of the Sun. These entities, bounded by outlines which blend almost imperceptibly into the general light of the corona, are deserving of especial study, which will be best accomplished by means of photography, as it truly records their faint contours, which are likely to be overlooked in eye-observations, because they are lost in the softened light which surrounds the Moon; and also because the eye is naturally attracted most by the prominences which have a distinct outline.

The wire of which Major Tennant speaks in Photographs Nos. 1 and 2 can be well discerned in the first, but is seen with difficulty in the second picture. In none of the paper copies can the Moon's limb be traced beyond a little more than the half of its circumference;* and, at first, it did not appear possible to discuss these pictures graphically in the same way as I had done my own photographs of 1860, but ultimately I was enabled to make out the diagram which I now lay before the Society; the work was accomplished in the following manner:—

By very careful measurements the centre of each picture was first found, and a circle representing the lunar disc then described with the dry point of the compasses.

Taking the angle of position of the Great Horn (which did not change greatly during the totality†) as 75° in all the pictures, the line N S was ascertained and also drawn with a dry point. A careful tracing was then made of the pictures 1, 2, and 6, and the tracings were in such a manner superposed that the Great Horn coincided in all, and that the lines N S, N' S', N'' S'', were seen to be as nearly as possible parallel. By these means the relative positions of the Moon's centre at the epochs of No. 1 and 6 were obtained. By drawing the circle to represent the Sun in such a position that the lunar peripheries at epochs Nos. 1 and 6 were tangential to it, the radius of the Sun was ascertained and the position of his centre at the same time laid down. It was then possible to obtain the following elements, as may be seen by an inspection of the diagram, which is a reduction, to about the same size as my original photographs of 1860, of a larger diagram, drawn out on a scale of half an inch to a minute of arc:—

Orbital motion of the Moon's centre during the totality, referred to the Sun, was a parallel to a diameter drawn from	..	..	..	285° to 105°
Orbital angle therefore	..	..	..	15°

* In the glass copies the Moon's limb can be traced nearly all round in photograph 5, and completely so in 6.

† This angle changed about 2° 50' in reference to the two positions of the Moon's centre in photographs Nos. 1 and 6; but this was only ascertained as the diagram was developed; for this reason the lines N S and N'' S'' could not be made quite parallel when tracings of the photographs were superposed.

Measured orbital motion during the interval between Nos. 1 and 6, namely $5^m 31^s.9$, reduced to seconds of arc*

132''

The relative motion of the Moon's centre in Right Ascension during the interval between Nos. 1 and 6

W. to E. 128''

The relative motion of the Moon's centre in Declination during the interval between Nos. 1 and 6

S. 32''.4

[The relative motion *during the totality*, namely in $5^m 45^s$, would consequently have been:— in R.A. $133''.5$ and in Decl. $33''.7$.]

Position-angle of 1st internal contact, or point where the light disappeared

101°

Position-angle of 2nd internal contact, or point where the light reappeared

294°

Nearest approach of centres

7''

Ratio of the Moon's semidiameter to the Sun's semidiameter = 1

1'07188

Prominence.	Position Angle referred to Sun's Centre.		Sun's Limb.	Height above Moon's Limb.
A	76°0	81°5	3 10"	3 4†
B	123°0	127°0	1 0	0 54
C	139°0	148°5	1 32	1 19
D	262°0	263°0	0 22	0 13
E	283°9	287°5	0 54	0 54
F	295°5	298°5	0 44	0 44
G	314°5	322°0	1 7	1 0
H	326°0	331°0	0 45	0 32
I	335°5	337°5	0 48	0 27
K	65°0	72°0	...	0 40‡

In the diagram I have inserted the position of the Sun's spots and faculæ, as photographed at the Kew Observatory. §

Sun-spots do not appear to have had in August, 1868, more than in July, 1860, any immediate connexion with the prominences. A patch of faculæ, however, between position-angles 138° and 147° , extending up to the limb of the Sun, corresponds well with the position of prominence C. And the patch of faculæ 255° to 257° nearly so with the small prominence D.

Indeed, so far from anticipating that there could be any prominences surmounting Sun-spots or in immediate contiguity with them, the published researches in Solar Physics by myself, Mr.

* The value of the arbitrary scale employed was reduced to seconds of arc by taking the Sun's semidiameter to be $15' 50''.6$, and deducing therefrom the value of $1''$ on the scale.

† These heights are exclusive of the curved point *a*, fig. 7, which cannot be seen in the paper photographs; its length is about $12''$, making the total height of the Great Horn $3' 22''$ above the Sun's limb.

‡ Not shown in the diagram, as it is not visible in the paper copies.

§ *Monthly Notices*, vol. xxix. p. 3.

Balfour Stewart, and Mr. Loewy, induce us to think that there ought to be an absence of these appendages where Sun-spots exist. The prominences are evidently more closely connected with the faculæ, for we now know with certainty that they arise from an uprush of heated gases, whereas we have stated that Sun-spots are closely connected with a downrush of comparatively cold gaseous matter.

The positions of the Sun's axis, equator, and two parallels of latitude, 30° N. and 30° S. in the diagram, are derived from the following data:—

Inclination of the Sun's Axis to the East of the Meridian	°
through the Sun's centre	17 24.9
Elevation of Earth above Sun's Equator	+6 52.6

Major Tennant, in a previous letter, communicated to me his observations of the spiral structure of the Great Horn, A, which was printed in the *Athenæum*, October 3, 1868, No. 2136, with some remarks of my own, in which I gave the credit to Major Tennant for having first pointed out this peculiarity of structure; but, at the same time, I called attention to the fact that a similar spiral structure was visible in my photographs of the Eclipse of 1860, for example in the prominences C and D, as depicted in my plates Nos. ix. to xv. *Phil. Trans.* 1862. In his letter of November, 1868, Major Tennant adds the following statement:—"About the Horn: I trust there can be no doubt about its structure, but I would draw attention to the closeness of the spiral low down, when first formed, and its size above, which seem to me to accord with my theory. Indeed, I almost feel that there are two close spirals in the lower part of the luminous vapour, each partaking of the general motion, and in the upper part coalescing. There should be, according to Dr. Vogel's account, a sensible change in the $5\frac{1}{2}$ minutes, and I think I have mentioned that I suspected one before I saw his paper. It is quite clear that, from some cause, the latter photographs are taller than the first; what puzzles one is the white hole (Fig. 7 b). In No. 1 one sees the knob outside it; and in No. 6 a little of it is seen in the same place; as it is in all between these two pictures. At the time this seemed to me conclusive against a change, but I am disposed now to believe that this could be explained otherwise, and that the change of height is real, and not an accident of exposure entirely. My theory would require the change, but that was not formed until recently, long after my suspicion had been given up as too uncertain for evidence, and I did not hear of Dr. Vogel's result until after my paper was ready for dispatch."

The paper copies are not sufficiently distinct to enable me to ascertain whether there was any change in the aspect of the Great Horn during Major Tennant's observations,* but it is very

* From a cursory examination of the glass copies, I believe that they contain evidence of an axial rotation of Prominence A.—Jan. 23, 1869. W. D. L. R.

probable that there was, for there is reason to suspect that this prominence was undergoing rotation; and I call attention in support of this view to the very different appearances of prominence A depicted by various observers along the line of totality.

I am dependent in the main on the representations of the phenomena given in the *Engineer*, of November 6 and November 13, for the appearances presented by the Great Horn (A) at Aden, on board the ships Rangoon, lat. N. $15^{\circ} 42'$, long. $59^{\circ} 15'$ E., at Beejapoor, at Wha Tonne in the Malayan Peninsula, and at Labuan. My friend, Mr. W. Fane De Salis, one of the Directors of the Peninsular and Oriental Company, kindly sent me, in September, 1868, a tracing of Captain D. Rennoldson's drawings, and I find that two of them are faithfully represented in figs. 14 and 15 of the *Engineer*, and this fact inspires confidence in the other pictures published in that journal. Fig. 2 is a copy of one of the tracings sent to me.

On reference to the subjoined diagrams, all drawn out to the same scale as that representing Major Tennant's photographs, it will be seen that in the Aden photograph, obtained on Aug. 17, $15^h 30^m 39^s$, the Great Horn was so curved that its point was directed northward, see fig. 1; in fig. 2, representing the appearance observed on board the steam-ship Rangoon, the direction is even more decidedly northward; this observation occurred at $15^h 39^m 18^s$, or nine minutes after that at Aden. In fig. 3, as observed at Beejapoor at $16^h 1^m 32^s$, after a further interval of 22 minutes, the point is directed southward. At Guntoor, the extremity of the Great Horn still pointed southward, but not so markedly; the appearance of it is represented in fig. 4, and occurred at $16^h 10^m 52^s$. So that it is probable that the Great Horn performed half a rotation on its axis between the observations at Aden and those at Guntoor, that is, in 40 minutes. Fig. 5 shows the appearance of the Great Horn as observed on the Malayan Peninsula, at about 17^h ; in this case the extremity points northward, as at Aden, and it appears to have performed half a rotation in the interval between the Guntoor and Wha Tonne records, that is, in 49 minutes; and at Labuan, at $17^h 52^m$, the direction of the extremity is southward, and the prominence had possibly performed another half a rotation in 52 minutes, the velocity having progressively decreased. Between Aden and Labuan one-and-a-half rotation was apparently accomplished in a direction from left to right, looking from the Sun's centre towards the prominence.*

I am not aware whether the representation of the Aden observation in the *Engineer* was copied from an actual photograph or from a print which had appeared in another publication. The hypothesis which I indicate of the probable rotation of the Great Horn depends, however, very much upon the faithfulness of the woodcut in the *Engineer*, and until prints of the Aden photo-

* The direction of the rotation is inferred from that of the convolutions of the spiral of the Great Horn.

graphs are compared with those of Gunttoor it will not be possible to speak with certainty on the point, nevertheless I have thought it desirable to draw attention to the subject.

In Major Tennant's photographs, and in my own of 1860, the deep indentation of the concave side of the prominences is very striking, leading to the impression that they do not actually touch the photosphere, but are elevated at some little distance above it.* The interval must be extremely small, for in 1860 I did not notice any luminous ring between the Sun's limb and the prominences, nor was any depicted in the photographs; but at the recent eclipse there appears to have been remarked by more than one observer that there was an indication of such a brilliant ring of light. In Major Tennant's first photograph, between position-angle 80° and 120° , is a large halo (covering an area equal to about $\frac{1}{4}$ of the lunar disk), which prevented part of the surroundings of the Sun's limb from being clearly depicted, produced evidently by a strong light, and in reference to this he makes the following remarks in his letter of November 9, 1868:—

"I wish to call your attention to the bright edge of light in No. 1, which I believe to be the stratum of heavy luminous vapour. It may be said that there is a glare, and this may be the residual sun; but the cusps extend beyond the glare, and it would have been hidden only after 10 seconds, while Phillips says (what is confirmed by other testimony) that the Sun was lost very shortly indeed after the exposure. I am quite satisfied this was not the solar disk, and if there be any question about it, I hope you will be able to confirm my opinion."†

Under date of November 28, 1868, Major Tennant, in relating a conversation he had had with M. Janssen, says, "His account of the passage of the Moon's disk over the edge of the Sun puzzles me. As he puts it, the bright surface in my No. 1 must have been the Sun itself, but if so, the limb must have been very pale beyond all I ever saw. He, however, said he could not be certain that a narrow line did not exist, when I pressed him."

I cannot from the examination of the paper copies say with certainty whether the bright glare was or was not due to the residual Sun, but I rather inclined at first to the belief that it was not due to the Sun.† This opinion was founded on my experience of 1860, when a very minute crescent of the Sun was taken, in No. 28 photograph, when the versed sine of the Sun's cusp was only $9''\cdot6$, by an extremely rapid uncovering and recovering of the object-glass; the instantaneous slide in the Kew heliograph not having been brought again into action for the partial phases after it had been removed for the totality pictures, until the epoch of the next succeeding photograph. Although, in the case of the Kew heliograph, the image, in consequence of its enlargement by

* In the glass Photograph 6 are depicted several perfectly detached prominences which are not seen in the paper copies.

† A careful examination of glass photograph No. 1 has convinced me that the glare was not due to a cusp of the Sun.—Jan. 23, 1869. W. D. L. R.

the secondary lens, was reduced in intensity 64 times, yet the plate was so solarised that no trace of prominence A, which was visible at the time, was depicted; whereas in No. 1 of Major Tennant's photographs very minute bead-like prominences near the Great Horn, together with this and the prominences B and C, are clearly recorded.*

The position angle (namely $111^{\circ}5$) of the centre of the glare is $10^{\circ}5$ to the South of the first internal contact, and this halo could not have been caused by the last traces of a cusp of the Sun, which would have been to the *North* of the first internal contact, but a furrow on the Moon's limb may have allowed the passage of a minute beam of the Sun's light, and this would have been quite sufficient to have produced the glare, and I think this is a probable explanation of the appearance, but I reserve for a future communication my final opinion on this point.

In the account given by Capt. Haig, R.E., in the *Proceedings of the Royal Society*, No. 105, 1868, of his observations near Beejapoor, he gives the following extract from the notes of Capt. Tanner, who took part in them:—"As regards the corona, when we first began to see the Eclipse through the clouds, I was under the impression that the eclipse, instead of being total, was only annular, so bright was the corona near the Moon's limb. I could not detect any irregularities in the light of the corona, but the light appeared to be gradually shaded off all round." In this bright light are evidently two distinct phenomena mixed up together, namely, the brilliant self-luminous entities with softened outlines, before spoken of, and the true corona caused by the reflection of the Sun's light by his non-luminous atmosphere.

I quote from the *Engineer*, November 13, 1868, page 371, with respect to the observations of M. Stéphan on the Malayan Peninsula: "After the side of the Sun had disappeared the Moon was seen by M. Stéphan and M. Tisserand as bordered by a thick luminous ring for about a quarter of a minute; this ring was brighter than the surrounding sky, and was sufficiently brilliant to cause an error as to the exact moment of the contact. It reappeared a few seconds after (before?) the third contact."

Although the extent of the ring of light is evidently estimated at far too large a quantity for it to be situated beneath the underside of the prominences or to have escaped depiction in the photographs, and is, in all probability, due to the same causes as the phenomenon described by Capt. Tanner; yet it would appear that there are still some doubts left to be settled, and that, notwithstanding the means we now possess of studying the Sun's appendages when he is not eclipsed, we must wait for future total eclipses before we can ascertain with certainty whether there

* On the occasion of the reading of this paper, Serjeant Phillips was present, and he stated that in his opinion the glare might have been caused by residuary sunlight (for the signal to expose the plate was given to him before the Sun completely disappeared), and that he purposely delayed doing so for a few seconds, until he judged by the *unassisted eye* that the Sun had become covered.

TOTAL ECLIPSE OF THE SUN AUG. 17-18, 1868.

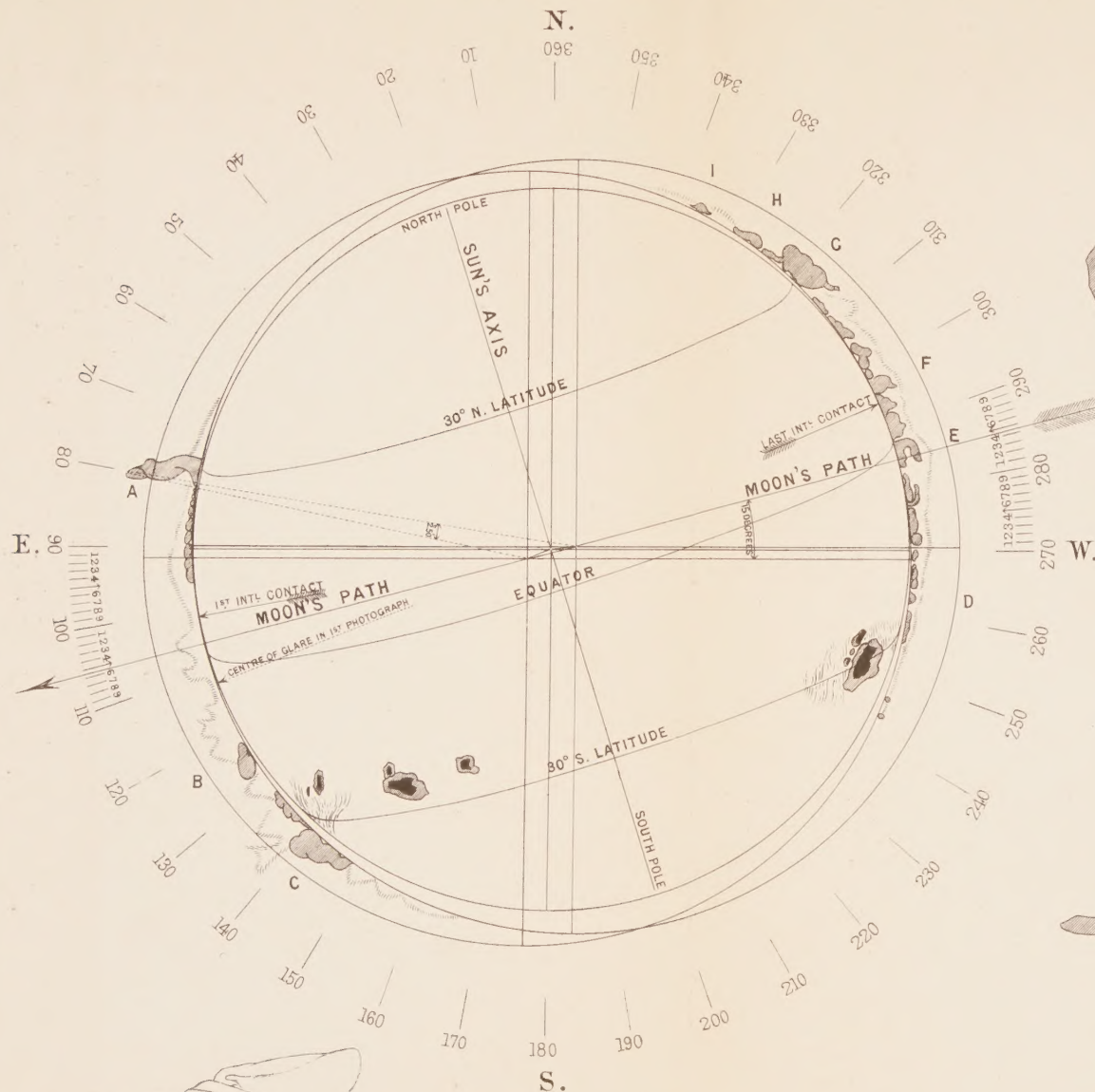


FIG. 7. PROMINENCE A.

FIG. 4. GUNTOOR.
17, 16^h. 10^m. 52^s.



FIG. 1. ADEN.
17, 15^h. 30^m. 39^s.



FIG. 2. SHIP RANGOON.
17, 15^h. 39^m. 18^s.



FIG. 3. BEEJAPPOOR.
17, 16^h. 1^m. 32^s.



FIG. 5. MALAYAN PENINSULA.
17, 17^h.



FIG. 6. LABUAN.
17, 17^h. 52^m. 30^s.

Interval.		
ADEN and GUNTOOR	40 ^m . 13 ^s .	
GUNTOOR " MALAY PENS.	49	
MALAY P. " LABUAN	52	30



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is any self-luminous atmosphere between the photosphere and the underside of the prominences.

It will, on future occasions, be extremely desirable to have photographic observations made as nearly as possible on both the Northern and Southern limits of the shadow as well as at stations situated near its centre.

Moreover, it would be very desirable to have two precisely similar instruments at the same station, in one of which the sensitive plates would be exposed for 1 second, and in the other instrument from 30 seconds to 1 minute, in order to obtain data for the correct interpretation of all the phenomena.

In connexion with the observations of the Sun's appendages when it is not eclipsed, it may be permitted to recall to recollection my estimates of the relative brightness of the prominences and the solar disk,* namely, that the former were in 1860 from 696 to 1111 times less brilliant than the Sun; I think this will be confirmed when the relative dispersion of the Sun's light and that of the prominences by the spectroscope is calculated; this of course depends on the dispersive power of the prisms and the opening of the slit. If, for example, the solar spectrum is dispersed over a space of 10° or $600'$, and the bright lines occupy together about $2'$, the light of the prominences would only be three times less brilliant (according to the mean of the estimates I have quoted above) than the Sun, and might in consequence be seen on the spectrum of the disk, which we know is possible; it would of course be much more brilliant than that of the spectrum of our atmosphere in contiguity with the Sun.

Mr. Hind has, since this paper was written, been so good as to give me the following data, calculated for the assumed position of Major Tennant's station:—

Guntoor.

Longitude	$80^{\circ} 27'$ East.†
Latitude	$16^{\circ} 15'$ North.
Middle of Eclipse, 1868, Aug. 17,	$16^{\text{h}} 10^{\text{m}} 52^{\text{s}}$ M.T. at Greenwich.
Duration of totality ..	$0^{\text{h}} 5^{\text{m}} 45^{\text{s}}$
Sun's semidiameter ..	$15' 50''.6$
Moon's apparent or augmented semidiameter	$16' 59'' 0$
Position-angle of first internal contact, or point where light first disappeared	103° N. towards E. direct image.
Position-angle of last internal contact, or point where light reappeared	71° N. towards W. direct image.
Nearest approach of centres	$3''.9$
Moon's apparent hourly motion in R.A.	$23' 27''.3$ E.
„ „ Decl.	$6' 32''.1$ S.

* From two different sets of considerations, *Phil. Trans.* 1862, p. 405.

† The position given in Major Tennant's paper is—

Long. E. $80^{\circ} 27' 9''$; Lat. N. $16^{\circ} 17' 29''$.

[Jan. 29, 1869. W. D. L. R.]

Moon's apparent Decl.	..	..	..	13° 3' N.
Orbital angle is therefore	..	..	..	15° 58'

A comparison of the data deduced from Mr. Hind's numbers and my own is subjoined:—

	Hind.	De La Rue.
Position angle of first internal contact, N. E. S.W.	103°	101°
Position angle of second internal contact ..	289°	294°
Nearest approach of centres	3''·9	7''
Moon's apparent motion during the totality in Right Ascension E.	134''·86	133''·5
Moon's apparent motion during the totality in Declination S	37''·59	33''·7
Ratio of semidiameter of Moon to semidiameter of Sun = 1	1·07195	1·07188
Orbital angle	15° 58'	15°

The data obtained graphically will be found to be not very discordant from those calculated, especially when it is taken into consideration that they were obtained from paper photographs on so small a scale as those at my command.

The Society has received from Major Tennant a detailed Report* of his Observations of the Solar Eclipse of 18 August, 1868, as seen at Guntoor, with illustrations, including the photographs and drawings referred to in the foregoing paper by Mr De La Rue: also the copy of a Report addressed to Sir Alex. Grant, Bart., Director of Public Instruction, Poona, by Mr. Kero Laxuman Chatrey, Prof. of Mathematics and Natural Philosophy at the Deccan College, of the Eclipse, as observed by him in company with Captain C. T. Haig, R.E., and Captain Tanner, at Beejapoor; the account by Captain Haig of these observations has been published in the *Proceedings of the Royal Society*. The Society has also received a communication from Col. T. Addison as to the observations of the Eclipse at Aden.

The Great Nebula round η Argûs. By Lieut. J. Herschel and Sir J. W. F. Herschel, Bart.

(*Observations of the Nebula by Lieut. Herschel, and Remarks by Sir J. Herschel, in reference to the supposed Variation of the Nebula.*)

Lieut. Herschel, in a letter to Sir J. Herschel, dated Bangalore, November 23, 1868, writes:—

* Jan. 29, 1869. I have this day had an opportunity of reading this most valuable and interesting paper, and am glad to say that my interpretation of the appearances depicted in the photographs agrees in many particulars with that of Major Tennant.—W. D. L. R.

I have this morning had a look at the Nebula round η Argûs. Had I known I could see it so early I should have tried sooner. It is now on the meridian (and therefore has an altitude of 20°) at sunrise, and I can see it readily two or three hours earlier.

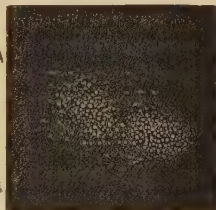
You will understand that I do not mean to content myself with one morning's examination, and that if I send you a hasty account, it is, in some measure, to record *first impressions* purposely.

The Nebula is *easily* seen with the naked eye. The eye *selects* the object as naturally as it would choose the Pleiades at that altitude. I have not compared it *directly* with Nubecula Major (preceding by six hours), but as I happened to have looked at the latter two nights before, I may trust my memory to say that the Nebula η Argûs is undoubtedly intrinsically brighter.

In a binocular it appears as two elliptic patches.

The figure is from memory.

I examined it telescopically with powers of 55, 85, 125, 200, sketching the appearance in each case. These sketches I have now before me. You know how difficult it is to represent faithfully one's impression of a nebula by a hurried pencil drawing, and will understand that the accompanying *copy** (based on all) has no pretension to accuracy. *In fact it is a wretched attempt.*



There is a copy (?) of your drawing in *Chambers's Astronomy*. This I had by me; and compared with my first and second sketches after they were made, not to correct my impression or sketch, but to fix the former and to note the agreements and disagreements in the latter. I did not look at your figure (in the *Outlines of Astronomy*) until I came in.

On the whole, sketch for sketch, I see no more marked differences than I should see in a similar comparison of a sketch of the Nebula *Orionis* with your figure (on the same page).

I marked the positions of twenty-nine stars in the central part only, of 9 to 12 or 14 mags. Of these I recognized the positions of seventeen at least, as practically identical with those in your figure, four are smaller than the run of those you show, three are so close to η that the brilliancy of that star no doubt obscured them. The remaining five are unidentified and unrepresented. η itself I estimate (ignorantly) at 7 to 8 mag.

I do recognize differences of configuration, no doubt, chiefly in the bridging of the dark space, and partial filling up of one portion; also the presence of another space scarcely more than indicated in your figure.

Upwards of fifty Nebulæ of different degrees of brightness

* Afterwards referred to by Sir J. Herschel as the sketch marked A: see Plate A.—ED.

have been under inspection, during the last few nights, and I think I may say that they have to some extent qualified my eye to judge of shape and relative brightness; and lastly to state that, so far as an hour and a half's study of this particular one is concerned, I do not recognize such a total "subversion of all the greatest and most striking features &c.," as Mr. Abbott's diagrams and statements appear to imply.

No doubt the relative brightness of some of the stars has altered; but, so far as I have compared, not remarkably (except η of course).

I propose to make a more precise survey of both stars and Nebula at once.

Sir J. Herschel, in a letter to Admiral Manners, dated Colingwood, Dec. 22, 1868, communicating the foregoing, writes:—

The extreme interest created by the account communicated by Mr. Abbott of the changes which would seem to have taken place in the great Nebula about η *Argûs* leads me to conclude that it would be wrong to delay communicating to the Royal Astronomical Society a letter which I have just received from my son, Lieut. J. Herschel, whom I requested to examine it so soon as its time of meridian passage would allow, with a view to ascertain, so far as the instrumental means at his command would admit, the nature and extent of the alleged changes, and at the same time to note the configuration of the stars surrounding the principal one (η). The letter is accompanied by two sketches—eye-drafts—the one (that marked A) being the one referred to more especially in his letter; the other (marked B) is not therein expressly referred to, nor indeed have I any positive ground for certainty that it is a sketch of that Nebula at all, further than that he speaks of having made sketches with each of the powers used, of one of which, probably that with a low power, I suppose it to be a copy; though it presents so little resemblance to the other as to allow of a very reasonable supposition (in the absence of any such guiding circumstance) that it had been taken for some other nebula. Indeed I should not have inclosed it at all, but as an instance of the very different impression one and the same nebula may make when viewed with different optical powers.

As the telescope with which the sketch (A) was made inverts differently from the 20-feet reflector with which my drawings were made, I annex a semi-reversed copy of (A) [marked (C)],* formed by pricking through the stars of (A), and then reversing the paper, and copying in with as much precision as possible the nebulous surroundings (semi-reversed) on the back of the paper. I have also added allineations pointing to the stars unmistakably identified, twenty in number, annexing the letters and numbers

* The semi-reversed copy (C) is not here reproduced; but see the plate (AA), which exhibits a like copy of Lieut. Herschel's second sketch of the Nebula.—ED.

which stand attached to them in my Catalogue of the Stars of the Nebula. Of these I ought to mention that two, viz., (α) No. 736 and (γ) No. 741, whose magnitudes are rightly stated at 9 and 11 in the Catalogue, are represented much too small in the engraving, though in their right places.

Unidentified are the four stars which form a pentagon with H towards η . There are eight small stars in that area in my chart, and these are doubtless four of them not laid down with sufficient precision for the purpose of identification.

The unidentified star marked $??\delta$ is a very important one. If it be really that star (a star 10 mag.) the evidence of change in the Nebula would seem to be really conclusive, since that star, when I delineated the Nebula, was carefully laid down as a landmark for future reference, as being just within (by a distinct though minute interval) the sharply-defined southern border of the Lemniscate, while three other exceedingly minute stars, with a similar view to future reference, on the south preceding border, were *just without* or *just on* the edge. But the allineations of the hypothetical δ disagree very markedly with those of the real δ , which lies in an exact straight line between C and (ζ), as also between D and G. The former condition is pretty well satisfied, but the latter is very wide of the mark.

On the whole, allowing for difference of optical power, there is no difficulty in recognizing the Lemniscate form, and my son's drawing, at all events, is decidedly adverse to the utter breaking up of its well-defined southern border by two clear open channels, which forms so marked a feature in Mr. Abbott's delineation.

Further and more elaborate delineations will undoubtedly follow.

P.S. On further consideration I am disposed to consider the sketch (B) as an attempt to delineate the clustering stars intermixed with nebula in the south preceding branch of the Great Nebula.

Lieut. Herschel's further observations, dated Bangalore, 29 Nov. 1868, are as follows:—

I examined this Nebula on the mornings of the 22nd and 23rd November with the Royal Society's 5-inch refractor, using different powers from 55 to 200, and sketching the appearance presented in each case. The second morning was devoted to one careful survey of the stellar points in and about the central portion, and to one sketch, based on it, of the nebulous light. The accompanying sketches show the appearances under powers of 55 and 125, both being drawn *afterwards* from the sketches made on the spot (the originals being retained, as I may be enabled hereafter to secure further observations). I believe they are both very faithful, but the appearances are so different, not only under different magnifying powers, but also under a more or less sustained gaze, that it is difficult to avoid giving features too marked a character,

on the one hand, and to indicate them correctly on the other. The relative intensity of the broader features is no doubt best seen under a low power, but the fact that in that case marked outlines are not traceable, which come out stronger under a high power, seems to indicate a tendency to exaggerate the latter effect. I am inclined to attribute somewhat of the differences *in this respect* between the present sketch and the figure of this Nebula in the "*Outlines*" to an endeavour on my part to avoid the difficulty by giving an intentional indistinctness to some parts about which I could not feel any confidence. It is hardly necessary to say that I carefully avoided comparing the above until I had completed my own.

Out of about sixty stars plotted, I believe not more than five or six are erroneously placed (to the extent of affecting appreciably the configuration). Of the rest by far the greater portion are easily identified with stars of similar magnitudes in the older drawing; a few are strangers, and perhaps two or three of these are (now) of sufficient magnitude to attract attention as not represented in the published diagram. In one or two cases also where there are *pairs*, their direction is more or less different; but this is hardly more than might be expected in thirty years.

The nebulous area, of which the accompanying low-power sketch forms the principal portion, is visible to the naked eye as a nucleus in the milky way. The latter appeared to me, on the morning of the 24th November, as of more intrinsic brightness than *Nubecula Major*, height for height; and of course therefore there was no comparison between the latter and the Nebula. It is an object which the eye selects at once for closer examination. Under a low power (of 3 or 4) it looks like two oval patches with many stellar points. Of these only one is shown in my sketch (2), being the portion which was naturally examined first.

There does not appear to be any very remarkable change in the distribution either of the stars or the Nebula, so far as that part immediately round α is concerned; but, if that portion formerly represented all that could be recognized in a moderate telescope, a great change must be acknowledged, arising, as it would seem, from the increased relative intensity of surrounding parts, rather than from subversion in the central.

It seems only necessary to add that the altitude at the times of observation was about 15° , and that the air was clear enough to have shown stars of the third magnitude within a degree of the horizon, roughly speaking.

Sir J. Herschel's letter, dated Collingwood, 2 Jan. 1869, communicating the foregoing, is as follows:—

Herewith I have the honour to enclose two drawings which I have received from my son, Lieut. J. Herschel, being those, I presume, alluded to in his first brief communication as intended

to be executed with greater care; as also a description accompanying them, which, if not too late for communication to the Royal Astronomical Society at their next meeting, might very advantageously be considered simultaneously with the former drawings and their accompanying comments.

I accompany my son's drawings with two others, which will serve the purpose of elucidation. The first, marked (A A), is formed by pricking off the stars laid down in his figure No. 2, of November 24, through the existing pinholes in this figure; and shows the direction of the meridian and orientation in that figure as it must have been at the time of making the sketch. It exhibits also the letters and numbers in my Catalogue of the stars in and near the Nebula with which those in his drawing are identified. Of all the stars so pricked off, forty-nine in number,* there is only one of whose identity there can be any doubt, viz., a minute star indicated in both our figures by a query ? attached. It is either the star 14th magnitude, marked as τ' , No. 792, in my Catalogue considerably misplaced, or the star No. 650, of 17th magnitude, with which its place agrees. If his (?) is intended to express a doubt as to the actual existence of a *suspected* star in that place, the latter supposition must be adopted.

The other accompanying chart, marked (B B), is formed by pricking off all the stars in my large engraving within the compass of the paper, and then, at the back of the paper so pricked through, lettering and numbering in ink the identified stars, and those only. The semi-reversion thus effected brings the stars of this chart (when placed with the pencil line drawn through η Argús and O parallel to a line through the same stars in Lieut. Herschel's figure) into correspondence as to right and left, top and bottom, with the rest of the stars in that figure, and their agreement (allowing for the difference of scales), will at once be evident.

On this chart I have also drawn in pencil the leading lines of the Nebula as it existed in 1834-38, from which I think it will be very evident (allowing for the great difference in the illuminating powers of the instruments employed) that the Lemniscate *still exists* and holds as nearly as might reasonably be expected under the circumstances the same relative position among the stars, and more especially as relative to η Argús itself. In this drawing of my son's it should be noted that the *true* (δ) makes its appearance quite unmistakably, and it stands just where it ought, a little beyond the border of the Lemniscate *within the Nebula*. It is true that the outline of the Lemniscate is shown much less sharply defined, and, holding the drawing at a distance and in a good light, it may be clearly traced. The star (α), too, I may observe intermediate between (5) and (6), has by some inadvertence been

* In his description he speaks of "about 60" originally "plotted." I can find, however, only 50 in this figure, one of which accidentally lies beyond the border of the paper in my projection.

omitted, but is unequivocally shown in its proper place in the drawing previously sent.

A decisive proof of the absence of any material shifting of position between the Lemniscate and η *Argús* is afforded by the small star marked [*v*] in a direction pointing from η to about half-way between C and D. This star (No. 664 of my Catalogue) occupies, both in my engraving and in my son's drawing, precisely the same strongly-marked position close to the very edge of the most characteristic projection of the Nebula, being *just immersed* in the Nebula in both.

The drawing No 1, with a low magnifying power, it should be observed, is on a scale only about one-third that of my engraving, and of course is intended to take in not the Lemniscate region only but the great diffused outlying masses. These, as will create no surprise to those accustomed to the difference of appearance in Nebulæ under great differences of illuminating power, offer hardly any recognizable similarity to the curious and complex detail exhibited in my engraving. Yet the Lemniscate, as a small *closed* form to the left of the small star (η *Argús*, the middle one between the three large stars laid down), is perfectly recognizable, though much more feebly marked than I should have expected. Of these three stars I presume the two upper ones in the figure to be W and Y of my chart. That in the lower portion is beyond its limits.

The plates belonging to the foregoing paper are —

1. Sketch A, in Lieut. Herschel's first communication.
 - 2 and 3. The drawings 1 and 2, in Lieut. Herschel's second communication.
 - 4 and 5. The tracings AA and BB, referred to in Sir J. Herschel's second letter.
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Description of an Automatic Transit Instrument.

By Sidney B. Kincaid, Esq. (Abstract.)

It is stated that the object of the paper is to describe an apparatus for the adjusting of isolated timepieces, so simple in its construction as neither to be very expensive to provide at first, nor easily liable to derangement when fixed, and so simple in its mode of use, that it might be mastered in the course of an hour by any person of moderate intellectual powers.

The apparatus consists of a plane mirror and a burning-glass to be adjusted in such manner that, at the instant the Sun reaches the meridian of the locality, the rays ignite a thread which burns

without smoke or residue; this releases a detent, and a motion is thereby given to the hands of the clock, bringing it to the correct local mean time. There is a supplementary arrangement by which, if, in consequence of a passing cloud, the thread does not ignite at the instant of the transit of the first limb, the subsequent ignition of the thread will not affect the clock.

The Transit of Mercury of Nov. 4-5, 1868. By C. Todd, Esq.

(Extract from a Letter to the Astronomer Royal.)

The weather being fine and beautifully clear I was able to take the following observations of the transit of *Mercury* on the 5th instant, which may perhaps be useful for purposes of comparison. I had previously calculated the angle from vertex of ingress, and looked for the planet at the right point; the time of first contact was, however, late a few seconds. The time of internal contact at both ingress and egress may be taken as correct, although the Sun was low down and rather ill defined at the egress. The observations were taken with a good Dollond's telescope of $2\frac{1}{2}$ -inches aperture.

Ingress.

	h	m	s	
a. First contact	2	35	20	Adelaide Mean Time.
b. Internal contact	2	41	30	,,

Egress.

c. Internal contact	6	13	53	,,
d. Planet bisected	6	14	59	,,
e. External contact	6	16	3	,,

- a. Seen late. Sun's limb slightly indented.
- b. Good.
- c. Considered good, but limb uneven.
- d. Only approximate.
- e. Sun's limb seemed perfect, but time may be few seconds too soon, the Sun being low and ill defined.

Ellery only got ingress.

I have made a good galvanic determination of the difference of longitude between Adelaide and Melbourne. I have fitted contact springs to my transit clock, and converted an ink-writing Morse instrument, by Siemens, into a very good Chronograph.

*Electric Telegraph Department,
Office of Superintendent,
Adelaide, Nov. 11, 1868.*

Note on Mr. Huggins' paper "On a Possible Method of Viewing the Red Flames without an Eclipse." By J. Norman Lockyer, Esq.

In the paper referred to I read as follows:—

"The observations of the eclipse of August last having shown the position in the spectrum of the bright lines of the red flames, Mr. Lockyer and Mr. Janssen succeeded independently by a similar method in viewing the spectra of these objects."

The obvious meaning of the paragraph is that my work was based upon, and that my success was due to, the observations made during the eclipse in India.

I therefore beg permission to show (I.) that my work was not based upon the Indian observations, and (II.) that my success was in no way due to them.

I. In the first place I may remark that my work was begun in 1866, and the method I adopted was clearly indicated in a paper communicated to the Royal Society in that year. My observations were then unsuccessful owing to the small dispersive power of the instrument employed, which I have since ascertained was utterly incapable of showing the bright lines.

Secondly, I attempted to remedy this state of things by using an instrument of greater dispersive power, and I obtained a grant for the construction of such an instrument, which was ordered in January, 1867.

Thirdly, after a long series of unfortunate delays, due to instrument-makers and to my own severe illness, the instrument was received in an incomplete state on the 16th of October last (Friday night).

Fourthly, the discovery of the prominence spectrum was made on the 20th of October (Tuesday morning), *i.e.* four days after the instrument was received, and on the very first day I gave to the observations after the instrument had been mounted, adjusted, and tested.

Fifthly, my method of observation was to clamp my telescope on a particular part of the Sun's limb, and then to sweep along the spectrum very slowly (three adjustments being necessary) from red to violet, then to clamp on another part and work back, and then to begin again.

II. We now come to the second point. In what I have hitherto stated I have shown that the coincidence in time between my own results and the receipt of the information from India *was due to the date on which the new instrument was received.* I am convinced that had the new spectroscope been received a year ago the discovery would have been made just as easily; and Father Secchi's remarks on the ease of the observations, even with an instrument of moderate dispersive power, strengthen this view. I have also stated my method of work.

Now let us see how the Indian observations were the cause of my success, or if indeed they could have caused the success of any one who depended upon them.

On the 20th of October I knew of three sets of observations : Rayet's, Herschel's, and Tennant's.

Rayet gave	B	D	E	b	F
	and four more undetermined lines.				
Herschel gave	{ near B or C }		D		{ near F }
declaring against F,	and hesitating very much to assign even an approximate place to the line in the red.				
Tennant gave	C	D		b	{ near F }
	believed he also saw a line near G.				

The above table will, I think, show how utterly useless the Indian observations were for the purpose Mr. Huggins has assigned to them. In fact, every lettered line, except A and H, is named. Had I indeed worked with them at all I should naturally have looked for the line D, as that is the only line in which all the observations correspond. I distinctly referred to hydrogen (= C and F) in my paper of 1866, but in 1868 only one observer is sure of C, and only one observer is sure of F, and now it is stated that my success is due to the Indian observations.

I have sent this paper to the Royal Astronomical Society, not because any credit due to M. Janssen and myself is lessened by Mr. Huggins' paper, but because I know from experience that its *Monthly Notices* are one of the most important sources from which the history of Astronomy is written. I naturally therefore prefer that a true account of the recent discovery should appear in the *Monthly Notices*, instead of a statement incorrect in fact, which, were it left unnoticed, would mislead those who come after us.

On the Relation of the Luminous Prominences to the Faculae of the Sun. By Prof. E. W. Brayley.

In the *Monthly Notices* for November 13, 1868, is inserted a paper on the Sun-spots and general aspect of the Sun on the day of the total eclipse of August 18, communicated by Messrs. De La Rue, Stewart, and Loewy. The large groups of spots visible, one near the eastern, the other near the western limb, were connected with faculous matter which apparently extended beyond the visible disk on both sides. The angles of position of these two parts having been given, the authors remark, "How far these

positions agree with any observed protuberances can only be gathered after some time, when the whole of the detailed accounts have come to hand."

Mr. De La Rue does not refer, on this occasion, to a similar observation of his own which is recorded in his Bakerian Lecture on the Eclipse of 1860. With the view of ascertaining whether any connexion exists between the luminous prominences and the faculæ, or the spots on the solar disk, he obtained photographs of the Sun on the only five days, from the 14th to the 20th of July, on which the state of the weather permitted.

The faculæ surrounding a certain group of spots "extended evidently," Mr. De La Rue states, "beyond the visible portion of the Sun's surface on the 18th; for a part which was not in sight on the 18th came into view on the 19th and 20th. Just in the neighbourhood of these faculæ there was visible in the telescope during the totality *a very brilliant sheet of light*."* The significance of this last fact will appear hereafter.

The consideration of the facts recorded in the paper by the Kew observers reminded me of some observations cited in a *résumé* of recent researches on the Sun contained in three papers on its physical constitution which I contributed to the *Companion to the Almanac* (Knight's) for the years 1864, 1865, and 1866. Among these are some of great importance in relation to the present subject.

We have evidence that the terms *luminous prominences* and *faculæ* merely designate different portions, the inferior and the superior respectively, of the same physical objects. For this we are principally indebted to the photographic researches of Mr. De La Rue, the results of which I shall proceed to interpret.

In the first of the papers just alluded to, inserted in the *Companion*, for the year 1864, p. 45, I stated that the prominences might be regarded with some probability as arising from the exterior flames and condensed vapour or smoke "of the faculæ or their crests;" though I had not then recognised the force of the evidence on the subject which existed even at that time.

Mr. De La Rue had inferred from his own observations, both telescopic and photographic, of the prominences as seen in 1860, that they are far more generally distributed on the solar disk than the spots, being indeed very widely scattered over the Sun's surface. I afterwards stated (*Ib.*) "We must infer from the great extent of the limb they occupy, in profile or section, that they extend over the whole surface of the Sun." I did not mean as a continuous stratum (to which idea I have no claim), but merely as a collection of detached clouds, as they were then assumed to be, or detached ranges of clouds in the same plane as it were, forming a discontinuous spherical shell above the photosphere and the absorptive composite atmosphere incumbent upon it.

* *Phil. Trans.* 1862, pp. 395-396.

By placing photographic pictures of portions of the Sun's surface in the stereoscope, Mr. De La Rue ascertained that the faculæ, as he inferred, occupy the highest positions of the photosphere; and "in one case," he relates, "parts of the faculæ were discovered to be sailing over a spot, apparently at some considerable height above it."*

Now the first of these results agrees with the fact that the luminous prominences are the most exterior phenomena of the Sun which are seen upon or above the limb; the second exactly describes the appearance which a floating prominence would have in a photograph viewed by the stereoscope; the actinic power of the prominences in general having been found by Mr. De La Rue to be very great. The prominences are the only phenomena of the Sun otherwise known which are seen detached from the edge of the disk. This result agrees also with the facts that some of the prominences are detached, and at a distance from the edge of the disk equivalent to the altitude of many thousand miles above the photosphere, which we know to be attained by many of the prominences which appear to be still connected with the edge.

I have stated, in the first article on the Sun already referred to, after Mr. De La Rue, that he entertains the hope of obtaining the outline of the prominences ("in plan, as it were, in solar horizontality," I have added for the sake of perspicuity), "'as very delicate dark markings,'" now to use his own words, "'on the more brilliant mottled background of the photosphere,' which, by the use of the stereoscope, will be discriminated from the other markings of the Sun's surface, and appear in their true position." It is remarkable that Mr. De La Rue did not recognise the fact that the photographic observation of the floating facula already cited was in reality an example of the fulfilment of this hope. What other appearance could one of the detached luminous prominences when thus observed present? It might indeed have been continuous with the spot over which it was seen, if the intervening matter were transparent, and that this was actually the case is manifest from the observation itself.

We have thus the strongest evidence that the faculæ and the luminous prominences are identical, or at least that the latter are the superior terminations of the former. If such be the fact, they must also be connected with the spots. I shall endeavour to evince, in another paper, that it will be consistent with all known facts to believe that they are the prolonged summits, rising from the level of the photosphere, of the torrents of matter originally wholly gaseous, ascending from the nucleal regions of the Sun, which, piercing through the successive envelopes of the nucleus from the cloudy stratum to the photosphere, cause the sun-spots; that the observed height to which the prominences ascend is at once the consequence and the index of the enormous force with which the original torrents are projected from the nucleus; and

* *Phil. Trans.* 1862, p. 407.

that these inferences are not inconsistent with the local character of the spots.

London Institution, Jan. 8, 1869.

Addition, January 23, 1869.—Since the reading of this paper, it has been objected that, in consequence of the brilliancy of the photosphere exceeding by some hundreds of times that of the luminous prominences, while it is itself exceeded by that of the faculæ, the phenomenon observed by Mr. De La Rue—"the bright appendage of the Sun," which sailed "over the spot"—could not have been a prominence.

In the paper above announced, however, I shall adduce reasons, founded on Mr. De La Rue's own data, combined with the evidence of the spectroscope, for believing that in the stereoscopic projection of photographic pictures of the Sun's disk, the prominences, notwithstanding their inferior intrinsic brilliancy, may be expected to be seen as *bright* and not as *dark* markings, in agreement with my interpretation of the photographic appearance in question.

It has also been objected that the luminous prominences cannot be even partially identical with the faculæ, because the continuous spectra of the latter, forming part of the solar spectrum, proves them to consist, not of æriform, but of solid or liquid materials (probably in the state of incandescent cloud), while the prominences have now been shown to be simply gaseous.

To this I shall only reply, at present, that, at the temperature of the photosphere, the faculous matter (especially if it consists of solid or liquid hydrogen), must be perpetually undergoing resolution into the æriform condition at its upper surface, and, therefore, that while the spectrum of the lower parts of the faculæ-prominences will be continuous, that of their upper portions will be monochromatic, in accordance with the observed facts. But to this subject also I shall return in the sequel.

A communication has been received from Comm. D. E. Ashe, Director of the Quebec Observatory, stating that, since the date of his paper, "On the Physical Constitution of the Sun," *Monthly Notices*, vol. xxvi., p. 61, he had had his Equatoreal (aperture 8 inches, focal length 9 feet) fitted for Celestial Photography by merely inserting a sheet iron tube, in which is fixed the tube of a "Voigtländer" Camera, giving a picture of the Sun 3.4 inches in diameter, at a distance of 14 feet from the object-glass, and that the photographs are very good—two negatives sent to the Astronomer Royal showing all the phenomena of the surface, even the willow-leaves or granulations. The author's observations have confirmed him in the opinion, and he adduces further arguments

in support of the theory that the Sun-spots are planetary bodies of small dimensions, originally revolving between *Mercury* and the Sun, and which have fallen upon the Sun.

Comparisons of Sun-spot Observations made in Dessau and Kew Observatory in the year 1868.

Communicated by Messrs. De La Rue, Stewart, and Loewy.

Months.	Dessau.					Kew Observatory.				
	Number of New Groups.	Numbers of the Groups.		Days of Obs. Days without Sun-spots.		Number of New Groups.	Numbers of the Groups.		Days of Obs. Days without Sun-spots.	
January	2	No. 1	to No. 2	19	10	2	No. 787	to No. 788	6	4
February	2	3	4	21	2	5	789	793	9	2
March	8	3	12	27	0	8	794	801	12	0
April	8	13	20	25	0	6	802	807	12	0
May	7	21	27	31	1	5	808	812	21	0
June	9	28	36	28	2	15	813	827	19	2
July	8	37	44	31	7	8	828	835	21	4
August	8	45	52	31	0	7	836	842	16	0
Sept.	13	53	65	30	1	13	843	855	12	0
Oct.	13	66	78	28	0	17	856	872	16	0
Nov.	11	79	89	19	0	8	873	880	7	0
Dec.	12	90	101	11	0	21	881	901	13	0
Total	101	No. 1	to 101	301	23	115	No. 787	to 901	164	12

REMARKS.

The numbers of the groups in the Kew list are in continuation of the Catalogue given by Messrs. De La Rue, Stewart, and Loewy, in their "Researches on Solar Physics," part i.

The cause of the discrepancy in the total result is the discontinuation of the observations in Dessau since December 15, in consequence of the illness of Hofrath Schwabe. The latter part of December was especially rich in new and large groups, and there is every reason to believe that we are approaching rapidly the Sun-spot Maximum.

A very interesting phenomenon, viz. an indentation in the Sun's limb produced by a spot V, group No. 807, was for the first time simultaneously observed by the Kew and Dessau observers on May 7. The phenomenon was discussed by Messrs. De La Rue, Stewart, and Loewy, and the discussion has been ordered to be printed in the *Transactions of the Royal Society*.

*Kew Observatory,
Jan. 7, 1869.*

On the Nebular Hypothesis, and the Approximate Commensurability of the Planetary Periods. By Daniel Kirkwood, LL.D. Professor of Mathematics, Indiana University.

The views of Laplace in regard to the development of our planetary system have been confirmed in a remarkable manner by the recent progress of astronomical discovery. We have now (1868) 100 minor planets between *Mars* and *Jupiter*, of which 96 have been detected since 1845. The mean distance of *Flora*, the innermost member of the group, is 2.20; that of *Sylvia*, the most remote, 3.49. The breadth of the zone is, therefore, greater than the distance of the Earth from the Sun; greater even than the entire interval between the orbits of *Mercury* and *Mars*. Moreover, the *perihelion* distance of *Sylvia* exceeds the *aphelion* distance of *Harmonia* by a quantity equal to the interval between the orbits of *Mars* and the Earth. The Olbersian hypothesis of the origin of these bodies seems thus to have lost all claim to probability. The only remaining theory worthy of consideration is that of Laplace, proposed in 1813, soon after the discovery of the first member of the group. "The double fact," says Plana, "of the multitude of those bodies and of their circulation in the same direction around the Sun, is now too imposing to admit an explanation of their origin and formation different from that developed by Laplace, in his *Système du Monde*."*

The known facts, however, in regard to the zone of the minor planets, as well as the phenomena of *Saturn's* rings, seem to demand a modification of the Nebular Hypothesis as generally held. No reason has ever been assigned why the solar nebula should not have abandoned rings at distances intermediate between the present orbits of the planets. On the contrary, it seems highly probable that, after first reaching the point at which gravity was counterbalanced by the centrifugal force arising from the rotation of the contracting spheroid, a continuous succession of narrow rings would be thrown off in close proximity to each other, and revolving in different periods according to Kepler's third law. It would seem, moreover, as shown by Mr. Trowbridge,† that the solar nebula, during the process of ring formation, was very oblate, and also much condensed at the centre; while, according to Prof. Ennis,‡ the rotation of the nucleus was extremely slow.

The theory of an almost continuous separation of matter at

* *Note sur la Formation probable de la Multitude des Astéroïdes qui, entre Mars et Jupiter, circulent autour du Soleil.* Par Jean Plana. Présentée le 2 Mars, 1856, à l'Académie Royale des Sciences de Turin.

In a subsequent paper, by the same distinguished astronomer, it was shown that the mean inclination of the 62 minor planets known in 1861 was $8^{\circ} 25'$, exceeding the mean inclination of the solar equator to the ecliptic ($7^{\circ} 9'$) by only $1^{\circ} 16'$. This coincidence has been rendered still more close by later discoveries; the mean inclination of 100 asteroids being almost exactly 8° .

† Silliman's *Journal*, Nov. 1864, pp. 350-353.

‡ *Origin of the Stars*, section xix.

the equator of the solar spheroid is sustained by the fact that the zone of minor planets has evidently resulted, not from a single primitive ring, the parts of which moved with nearly the same angular velocity, but from *an indefinite number of narrow annuli*. The rings of *Saturn* may be regarded as another striking index to the process of planetary formation. With the exception of two permanent intervals, they appear to consist of small discrete masses, revolving in different periods according to their respective distances.

Why did not the Asteroid-zone form a single planet, and the rings of Saturn, one or more satellites?—In regard to the ring between *Mars* and *Jupiter*, perhaps no satisfactory answer can yet be given. We may remark, however, (1), that being situated just within the orbit of *Jupiter*, the perturbations were greater than in any other part of the system; and (2), that, as the total mass of the asteroids is very small, the matter of the primitive annulus was extremely rare, so that the intersection of orbits, resulting from the perturbations, would be less likely to produce large planetary nuclei. The rings of *Saturn* were doubtless comparatively dense. It has been shown, however, by Professor Vaughan,* that in positions so near the primary the force of gravity would render satellites of any considerable magnitude unstable. This force alone would, therefore, prevent the collection of large masses in the planetary form near the central body.

As these parts of the solar system, viz. the rings of *Saturn* and the minor planets between *Mars* and *Jupiter*, appear to furnish strong arguments in favour of the nebular hypothesis,† their phenomena may perhaps be further suggestive as to the *mode* of planetary formation.

The mean distance of a planet having—

A period equal to $\frac{1}{2}$ that of Jupiter is 3'2776			
"	$\frac{4}{9}$	"	3'0299
"	$\frac{3}{7}$	"	2'9574
"	$\frac{2}{5}$	"	2'8245
"	$\frac{1}{3}$	"	2'5012
"	$\frac{2}{7}$	"	2'2569

These distances all fall between the greatest and least mean distances of the asteroids. Now, do we find wider intervals in these portions of the ring than elsewhere? and if so, what is their physical cause? For the purpose of comparison we have, in the following table, arranged the minor planets in the order of their mean distances from the Sun.

* *Proc. Amer. Assoc. for Adv. of Sci.* 1856, p. 111.

† "No part of the solar system affords so striking an argument in favour of Laplace's Nebular Hypothesis as the Saturnian system of rings."—Proctor's *Saturn and its System*, p. 201. Plana's views in regard to the asteroids have been already quoted.

Order of Disc.	Name.	Distance.	Order of Disc.	Name.	Distance.
(8)	Flora	2'2014	(78)	Diana	2'6228
(43)	Ariadne	2'2034	(23)	Thalia	2'6250
(71)	Feronia	2'2654	(37)	Fides	2'6422
(40)	Harmonia	2'2677	(16)	Eunomia	2'6429
(18)	Melpomene	2'2956	(51)	Virginia	2'6486
(80)	Sappho	2'2971	(66)	Maia	2'6539
(12)	Victoria	2'3342	(26)	Proserpine	2'6556
(27)	Euterpe	2'3468	(55)	Io	2'6594
(4)	Vesta	2'3613	(73)	Clytie	2'6648
(30)	Urania	2'3642	(97)	Clotho	2'6661
(84)	Clio	2'3674	(8)	Juno	2'6687
(52)	Nemausa	2'3779	(76)	Eurydice	2'6698
(9)	Metis	2'3862	(77)	Frigga	2'6719
(7)	Iris	2'3862	(64)	Angelina	2'6783
(62)	Echo	2'3939	(34)	Circe	2'6839
(63)	Ausonia	2'3972	(58)	Concordia	2'6979
(25)	Phoebe	2'4023	(55)	Alexandra	2'7076
(20)	Massilia	2'4093	(60)	Elpis	2'7147
(67)	Asia	2'4209	(45)	Eugenia	2'7159
(44)	Nysa	2'4242	(38)	Leda	2'7399
(6)	Hebe	2'4254	(36)	Atalanta	2'7487
(63)	Beatrice	2'4287	(93)	Minerva	2'7554
(21)	Lutetia	2'4354	(73)	Niobe	2'7555
(42)	Isis	2'4401	(82)	Alcmene	2'7603
(19)	Fortuna	2'4414	(1)	Ceres	2'7660
(79)	Eurynome	2'4433	(41)	Daphne	2'7674
(11)	Parthenope	2'4526	(88)	Thisbe	2'7687
(17)	Thetis	2'4737	(56)	Pandora	2'7692
(46)	Hestia	2'5178	(2)	Pallas	2'7700
(80)	Julia	2'5498	(89)	Lætitia	2'7710
(29)	Amphitrite	2'5544	(89)	Leto	2'7748
(91)	Ægina	2'5584	(74)	Galatea	2'7777
(5)	Astræa	2'5772	(28)	Bellona	2'7784
(13)	Egeria	2'5775	(81)	Terpsichore	2'8563
(82)	Pomona	2'5831	(33)	Polyhymnia	2'8646
(14)	Irene	2'5895	(48)	Aglaia	2'8831
(47)	Melete	2'5976	(22)	Calliope	2'9091
(54)	Calypso	2'6101	(16)	Psyche	2'9263
(70)	Panopea	2'6132	(59)	Danae	2'9746

Order of Disc.	Name.	Distance.	Order of Disc.	Name.	Distance.
(68)	Hesperia	2'9949	(10)	Hygeia	3'1494
(35)	Leucothea	3'0060	(81)	Euphrosyne	3'1561
(96)	Ægle	3'0549	(90)	Antiope	3'1576
(95)	Arethusa	3'0829	(57)	Mnemosyne	3'1597
(50)	Pales	3'0861	(94)	Aurora	3'1629
(68)	Europa	3'0999	(92)	Undina	3'1917
(49)	Doris	3'1044	(76)	Freia	3'3877
(86)	Semele	3'1119	(65)	Cybele	3'4205
(61)	Erato	3'1309	(87)	Sylvia	3'4927
(24)	Themis	3'1420			

Remarks on the foregoing Table.

The mean between the distances of *Flora* and *Sylvia* is 2'8470; and as small bodies in the remoter part of the ring are more difficult of detection, the zone will be considered under two divisions. The inner section contains 72 of the 97 asteroids whose elements are known; the mean interval between them being 0'0081. The greatest gap in the order of distances occurs between *Ariadne* and *Feronia*. It is 0'0620, or nearly eight times the mean. This includes the distance at which seven periods of an asteroid would be equal to two of *Jupiter*. The chasm next in order of breadth is between *Thetis* and *Hestia*. This is 0'0441, or more than five times the mean. In the outer section the mean interval is 0'0286. The greatest hiatus is between *Undina* and *Freia*; the breadth being 0'1960, or more than eight times the mean. This includes the distance at which two periods of a planet would be equal to one of *Jupiter*. The second is between *Bellona* and *Terpsichore* (0'0779), and the third between *Leucothea* and *Ægle** (0'0489). The widest chasms in the zone of minor planets are thus found to include those distances at which the periodic times would be commensurable with that of *Jupiter*. These coincidences are not accidental. The *primitive* ring undoubtedly contained nebulous matter at these as well as the intervening distances; and the present existence of such intervals may be accounted for as follows:—

A planetary particle at the distance 2'5—in the interval between *Thetis* and *Hestia*—would make precisely three revolutions while *Jupiter* completes one; coming always into conjunction with that planet in the same parts of its path. Consequently, its orbit would become more and more eccentric until the particle would unite with others, either interior or exterior, thus forming the nucleus of an asteroid. Even should the disturbed body not

* We here except the most remote interval, that between *Cybele* and *Sylvia*, which is 0'0722.

come in contact with other matter, the action of *Jupiter* would ultimately change its mean distance, and thus destroy the commensurability of the periodic times. In either case the primitive orbit of the particle would be left destitute of matter. The same reasoning is, of course, applicable to other intervals.

The Rings of Saturn.

The equatoreal radius of *Saturn*, and the dimensions of the two bright rings, according to the measures of Struve,* De La Rue,† Main,‡ and Jacob,§ together with the mean of these measures, are as follows:—

	Struve.	De La Rue.	Main.	Jacob.	Mean.	In Equatoreal Rad. of Primary.
Equatoreal Radius of Planet .. }	8.9955	8.83	8.78	8.95	8.8864	
Exterior Radius of Outer Ring }	20.0475	19.915	19.865	19.995	19.9556	2.2456
Interior „ „ .. }	17.6445	17.665	..	17.910	17.7398	1.9963
Exterior Radius of Inner Ring }	17.2375	16.725	..	17.425	17.1292	1.9276
Interior „ „ .. }	13.334	13.455	13.825	13.135	13.4372	1.5121
The period a Satellite revolving at the distance } 1.9276, the interior limit of the interval, is }						h m s 10 52 11
One-sixth of the period of Dione is						10 56 53
One-third „ „ Enceladus						10 59 22
One-half „ „ Mimas						11 18 32
One-fourth „ „ Tethys						11 19 36
And the period of a Satellite at the distance } 1.9963, the exterior limit of the interval, is }						11 35 18

It is thus seen that the interval occupies precisely the space in which the periods of satellites would be commensurable with those of the four members of the system immediately exterior. As, therefore, the powerful attraction of *Jupiter* produces the observed *gaps* in the asteroid zone, so *the disturbing influence of Saturn's interior satellites is the physical cause of the permanent interval between the two bright rings.*

These facts and considerations are suggestive in regard to the numerous instances of approximate commensurability in the planetary periods. Let us suppose, in the first place, that at a former epoch the two innermost satellites of *Saturn*, *Mimas*, and *Enceladus*, constituted a broad, flat, and nearly continuous nebular ring, the separate portions of which revolved in different periods;

* *Mem. Ast. Soc.* vol. iii. p. 301.

† *Monthly Notices*, vol. xvi. p. 43.

‡ *Ib.* p. 35.

§ *Ib.* p. 124.

the matter of the exterior satellites having been previously collected into planetary forms. The first four satellites have the following periods:—

	d	h	m	s
Mimas	0	22	37	28
Enceladus	1	8	53	7
Tethys	1	21	18	26
Dione	2	17	41	9

The ring, therefore, from which the two innermost satellites were (hypothetically) derived must have contained (1) the orbit of a satellite whose period would be $22^{\text{h}} 39^{\text{m}} 13^{\text{s}}$, or one half that of *Tethys*; (2), that of a body revolving in $21^{\text{h}} 53^{\text{m}} 43^{\text{s}}$, or one third that of *Dione*; (3), that corresponding to a period of $1^{\text{d}} 8^{\text{h}} 50^{\text{m}} 34^{\text{s}}$, or one-half that of *Dione*; and (4), that of a mass whose period would be $1^{\text{d}} 6^{\text{h}} 12^{\text{m}} 17^{\text{s}}$, or two-thirds that of *Tethys*. The portions of this annulus at the distances named would be *unstable*, like the original matter in the interval of the present ring. The first two distances are nearly the same with that of *Mimas*; the third and fourth with that of *Enceladus*. The collision of masses and the formation of nuclei near the present orbits of these satellites would, therefore, result from the disturbing influence of *Tethys* and *Dione*. The remoter satellites afford similar instances illustrative of the same idea. Still others might be specified, but they will readily occur to every astronomer.

The degree of eccentricity attained by the orbit of such matter previous to its union, near one of the apsides, with other portions of the zone, would probably depend on the density of the original nebulous ring. This, in the case of the ring between *Mars* and *Jupiter*, is known to have been very small; and, accordingly, we find here the greatest eccentricities. It is obvious, moreover, that the nearest approximations to commensurability would generally be found in cases where the density of the primitive rings was the greatest.

The instances of nearly commensurable periods in the Jovian and Saturnian systems, as well as among the primary planets, have long been well known, and are too numerous and remarkable to be accidental coincidences. That the physical cause of such phenomena is a legitimate object of research will hardly be called in question. Hitherto, however, no explanation of the facts has been even attempted.

The foregoing view of the Nebular Hypothesis may not be necessary to account for the transmutation of rings into the planetary form. It assigns, however, an obvious cause for the establishment of nuclei in such positions that their periods will be nearly commensurable with that of the disturbing body. As these nuclei would receive accretions of matter from portions of space both interior and exterior to their respective orbits, their

distances from the central body, during their planetary growth, would not be liable to great variation.

On Aboul Hhassan's Catalogue of 240 Stars.

By E. J. Stone, Esq.

The interest attaching to our few ancient Catalogues of stars, is, perhaps, sufficiently great to excuse my calling the attention of the Society to a Catalogue of 240 stars, contained in the work of Aboul Hhassan Ali, translated from the Arabic by J. J. Sédillot. Paris, 1834.

The longitudes and latitudes are given for the commencement of the Hegira, July 15, A. D. 622. It is not known, I believe, from what sources these observations were drawn. Delambre, *Astronomie du Moyen Age*, p. 186, merely says, "Il nous donne un catalogue de 240 étoiles, pour le commencement de l'hégire, c'est-à-dire pour le 15 Juillet, 622. Nous l'avons réduit à l'époque actuelle; il nous a paru trop inexact pour être reproduit ici; les erreurs les plus ordinaires passent un degré. On y voit quelques étoiles australes que ne sont point dans le catalogue de Ptolémée." And, again, on the same page,—“Il donne les déclinaisons de 180 étoiles, pour le commencement de l'hégire, sans nous dire comment ces déclinaisons ont été observées. On peut croire qu'elles sont tirées des ouvrages d'Arzachel.”

I find more than 200 of these stars in Ptolemy's Catalogue. I have diminished all the longitudes of Hhassan's Catalogue by $6^{\circ} 40'$, and then compared the latitudes and diminished longitudes with the corresponding latitudes and longitudes of Ptolemy. The agreement is very close. The latitudes are identical; except in a very few cases indeed, and in nearly all these cases of apparent exception, one at least of the different readings given by Bailly will be found to agree with the latitude of Hhassan's Catalogue. Ptolemy's longitudes are only given to the nearest $10'$.

In some cases Hhassan's longitudes, as given by Sédillot, end with a $7'$, $8'$, or $9'$. In these cases we have respectively a discordance of $3'$, $2'$, or $1'$ between Hhassan's reduced longitude and that of Ptolemy. In nearly every other case we have identical results. The agreement, moreover, between the two Catalogues is such that in three or four cases where Ptolemy's longitudes are evidently 2° or 3° in error, relatively to the other stars, the same error is reproduced in Hhassan's Catalogue. I may mention that in the greater number of cases of discordance between the different versions of Ptolemy quoted by Bailly, Hhassan agrees with the copy marked L, "Venice edition, in Latin, by Liechtenstein, 1515," which is known to have been derived from Arabic sources. I am unable to account, in a manner perfectly satisfactory to myself, for the appearance of the slight discordances of $1'$, $2'$, or $3'$,

of which the discordance of $2'$ is by far the most common. These discordances are far too small to express the errors of observations of two independent catalogues. This will appear at once from a comparison between Ptolemy's Catalogue and that of Ulugh Beigh.

It has occurred to me that Ptolemy's places may have been laid down upon a globe and subsequently reproduced, for, perhaps, another epoch, without attention to the restriction of always taking the longitudes to the nearest $10'$. This would give rise to small discordances such as really exist between the catalogues. Whatever may be the true explanation of these small differences, I have not the slightest doubt about Hhassan's Catalogue being mainly a reproduction of that of Ptolemy.

There is one point connected with this Catalogue which appears to me to throw great light upon the origin of the belief which existed amongst many of the Arabic astronomers, of an oscillation in the precessional motion. Hhassan gives a Table for the calculation of the precessional motion in longitude in any time. His Table is founded upon the assumption :

$$\text{Precessional motion} = \text{Constant} \times \sin (5'409 t + 3^{\circ}51')$$

where t is the number of Arabic years from the commencement of the Hegira.

Hhassan's Table has been computed with natural sines to only two places of decimals, and these not always correct in the second place. It is therefore rather difficult to see the exact value of the constant which has virtually been employed. If, however, we take $610''$ for the value, the results agree closely with those given by Hhassan.

Hhassan quotes an observation of Hipparchus and another of Arzachel to show the accuracy of his result. He does not mention Ptolemy's Catalogue.

If we compute from the formula the precessional motion from the commencement of the Hegira to the epochs of the Catalogues of Arzachel, Ptolemy, and Hipparchus, we have

For Arzachel's Catalogue	$t =$	$+ 473$
Ptolemy's do.	$t =$	$- 500$
Hipparchus's do.	$t =$	$- 775$

Hence from the formula

Precessional motion to Arzachel's time	$=$	$+ 7^{\circ} 22'$
Ptolemy's do.	$=$	$- 6^{\circ} 42'$
Hipparchus's do.	$=$	$- 9^{\circ} 17'$

Hence between Hipparchus and Arzachel we have a motion of $16^{\circ} 39'$. Between Hipparchus and Ptolemy it is $2^{\circ} 35'$. Assuming the uniform precession equal to $50''$, we have for the increase of

longitude between Hipparchus and Arzachel, $16^{\circ} 45'$, agreeing very closely with that given by the formula. The precessional motion between Hipparchus and Ptolemy agrees with that assumed by Ptolemy, viz., $2^{\circ} 40'$. Hence the origin of Hhassan's formula appears to me clear: he could not reconcile the three great catalogues of Hipparchus, Ptolemy, and Arzachel, on the assumption of an uniform change of longitude. He adopted a simple formula which would reconcile these three catalogues, and, not doubting the accuracy of the observations, assumed that this formula must express a true law of nature.

If this view be correct, his belief in the oscillatory motion of the first point of *Aries* arose entirely from the systematic error—of about a degree—in Ptolemy's longitudes; and Ptolemy's want of candour respecting the nature of his Catalogue, is thus found to be throwing astronomy into complexities more than 1100 years after his death.

Measures of ζ Herculis. By G. Knott.

During the past summer I obtained the following measures of the binary star ζ *Herculis* with my $7\frac{1}{3}$ -in. Alvan Clark refractor, and a parallel wire micrometer by Dollond.

P=204 ^o 46	obs. 5	w. 23	D=0 ^{''} 951	obs. 4	w. 8	Epoch 1868 ^o 398
206 ^o 95	5	23	1 ^{''} 088	4	16	452
208 ^o 19	6	33	1 ^{''} 025	4	22	474
205 ^o 90	5	27	0 ^{''} 896	4	20	606

Mean results, allowing weight,—

P=206 ^o 53	obs. 21	w. 106	D=0 ^{''} 992	obs. 16	w. 66	Epoch 1868 ^o 48
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Considerable pains were bestowed on the distance measures from the fact that with the full aperture of 7.33 inches the image of the small star was projected almost centrally upon (perhaps very slightly *within*) the first bright ring surrounding the primary, thus affording a favourable opportunity for determining the radius of the first ring with that aperture. The result is in most close accordance with that given by theory. The radius of the first bright ring for a telescopic aperture of $7\frac{1}{3}$ -in. calculated from the numbers given by the Astronomer Royal on p. 80 of his *Undulatory Theory of Optics*, being 1^{''}009, which differs by only 0^{''}017 from the mean result of my own measures.

Woodcroft Observatory, Cuckfield, Sussex,
December 10, 1868.

Occultations of Stars by the Moon, observed at the Durham Observatory, 1867, November, 1868, May. By J. L. Plummer.

(Communicated by Prof. Chevallier.)

Day of Obs. 1867.	Phenomena.	Moon's Limb.	G.M.T.		
			h	m	s
Nov. 6	Disapp. of λ Aquarii	Dark	10	24	57.5
	Reapp. of λ Aquarii	Bright	11	33	52.2
	Disapp. of γ 8 Aquarii	Dark	11	57	18.6
Nov. 8	Disapp. of ι Ceti	Dark	7	10	6.1
	(a) Reapp. of ι Ceti	Bright	8	23	8.1
Dec. 11	(b) Disapp. of ι 30 Tauri	Bright	8	47	38.9
	(c) Reapp. of ι 30 Tauri	Dark	9	33	24.3
Dec. 16	Disapp. of ϵ Leonis	Bright	13	54	23.9
	Reapp. of ϵ Leonis	Dark	15	1	56.8
Feb. 4 1868.	Disapp. of ι 30 Tauri	Dark	6	55	8.6
Mar. 28	(d) Disapp. of γ Tauri	Dark	9	1	4.9
	Reapp. of γ Tauri	Bright	9	56	3.6
Apr. 9	(e) Disapp. of η Libræ	Bright	13	30	12.7
	(f) Reapp. of η Libræ	Dark	14	6	15.3
May 4	Disapp. of τ^2 Virginis	Dark	9	6	45.9
	Reapp. of τ^2 Virginis	Bright	10	17	42.5

(a.) Not exact; the Moon's limb unsteady. (b.) Not very exact; the star being faint from haze and the Moon only a few hours past the full. (c.) Not good; the star faint. (d.) The star was attached to the Moon's limb, which was distinctly visible by earthlight, at least 5 seconds before the disappearance, which was instantaneous. (e.) The star faint; the observation not very satisfactory. (f.) Light clouds passing, but the observation pretty good. (g.) Uncertain; the Moon's limb very unsteady.

Durham Observatory.

On a Defective Vision. By Major Tennant, R.E.

Some few members of the Royal Astronomical Society may be aware that I have occasionally complained of a somewhat defective vision. If I venture to enter into some details about it, it is not that the matter is of much importance as regards myself, but that I think many more are labouring under slight defects, and being unconscious of them, attach, in perfect good faith, a value to delicate observations which they do not deserve.

Many years ago, soon after I came to India, I found in look-

ing at the Snowy Ranges of the Himmaleh with a telescope that to my eye the snow had a reddish-brown tint. Shortly after I found that this was not the same for both eyes, but that, in passing from my right eye to my left with a telescope, I seemed to pass, as it were, from the brownish tints of autumn to the brilliant ones of spring. Every object became brighter and clearer, and the greens were especially affected. Close examination showed me that the vertical and horizontal wires in a telescope were never pleasantly seen together. One was always clearer than the other, and any occasional perfect vision passed away after the effort to see wore off.

I came to the conclusion then that there was really some defect in my eye. In 1857 and 1858 I was examining many of the double stars in Smyth's *Cycle*, and it struck me that I could not see blue stars two magnitudes larger than others there described which I did see. I also noticed that the very red stars described had never produced on me any marked impression of colour, and I deduced that my eyes were really somewhat insensitive to the blue rays.

So matters stood till last December, when I accidentally saw an optometer at Messrs. Horne and Thornthwaite's, which led me to ask Mr. Acland of that firm to examine my eyes. He came to the conclusion that I was somewhat short-sighted, and that the lenses of the eye did not refract as solids of revolution, but that a plano-cylindrical convex lens of 30 inches radius would nearly adjust my vision. I tried these, and, after finding the proper positions of the axes of the cylinders, found that the definition was enormously improved, and that the left eye was, as I thought, nearly perfect, and the right eye nearly as good as the left originally, but I was firmly convinced for some time that I was *not* short-sighted. At last, however, I became convinced that I was so with these lenses, and a spherical surface of the same radius as the cylinder was ground inside it. Now my vision was superior to anything before. On the voyage out I have often seen eight stars of the *Pleiades* (and notes of their places are in a note-book) where I never before saw more than five; but I found now I had become long-sighted, and could not use the glasses for reading, and became convinced that Mr. Acland's statement was correct, namely, that I was short-sighted, not because the near focus was short, but because I had a want of the usual range of adjustment and could not reach the distant one; when, therefore, distant vision was corrected the near vision became defective. I therefore made up my mind to have two sets of lenses.

But before ordering them I determined to re-examine my eyes with great care, and I now found, on using an achromatic glass as an eye-glass, and a peculiar object, that, in reality, the estimate I had made of the improvement in my right eye was very nearly right. I now found that the convex surface for the right eye required a radius of 20.5 inches, and for the left eye

one of 40, and that the concavity for distant vision required to be 27.5 inches in radius for each.

The red tint now remained to be accounted for, and I have been led to what I believe to be the true solution of that, and also of the insensibility of the right eye to blue light. I should mention that habit makes me see mainly with the right eye, that is, that its image clearly predominates in ordinary vision. I had noticed that while my general vision of stars was much improved with the spectacles I had brought from England, and the sky became dotted with small objects invisible without them, some stars certainly were less clear. It only occurred to me recently that on that occasion α *Scorpii* was injured and *Saturn* improved in definition by the glasses, and I then remembered that α *Scorpii* had generally been the defaulter, the extreme beauty of that constellation having always made it catch my eye. It is evident now that my right eye is not achromatic, that the red rays being naturally focussed predominate in vision, and that when the luminous image of a star is focussed the blue rays do not claim notice, but when a blue star is seen, if it be so small as to be near the minimum of brightness, its image is too diffused to be visible.

Now, my vision I used to think good. I have often at sea caught the masts and rigging of a ship before those searching for them, and the defects which I have spoken of I have frequently believed to be fancies, as they seemed inconsistent with many evidences that I had good vision. I should certainly have unhesitatingly trusted my eyes in any matter but colour. Possibly some of the strange discrepancies in description are due to similar defects of vision, and we may, some day, find it necessary to make every observer of delicate phenomena, who claims credit, *prove that he can see*.

Calcutta, Oct. 15, 1868.

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